NBER WORKING PAPER SERIES

EFFECTS OF THE MINIMUM WAGE ON EMPLOYMENT DYNAMICS

Jonathan Meer
Jeremy West

Working Paper 19262
http://www.nber.org/papers/w19262

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2013

We benefited greatly from discussions with Ronald Davis, Bethany DeSalvo, and Jonathan Fisher at the U.S. Census Bureau regarding the Business Dynamics Statistics, and from Jean Roth at NBER regarding the CPS-MORG. We are grateful to Jesse Cunha, Jennifer Doleac, Craig Garthwaite, Mark Hoekstra, Scott Imberman, Joanna Lahey, Michael Lovenheim, Harvey S. Rosen, Jared Rubin, Juan Carlos Saurez Serrato, William Gui Woolston, and participants at the Texas A&M Applied Microeconomics seminar, the Stata Texas Empirical Microeconomics workshop, and the Naval Postgraduate Institute for valuable comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the U.S. Census Bureau or the National Bureau of Economic Research.

Effects of the Minimum Wage on Employment Dynamics
Jonathan Meer and Jeremy West
NBER Working Paper No. 19262
July 2013
JEL No. J21,J23,J38

ABSTRACT

The voluminous literature on minimum wages offers little consensus on the extent to which a wage floor impacts employment. For both theoretical and econometric reasons, we argue that the effect of the minimum wage should be more apparent in new employment growth than in employment levels. In addition, we conduct a simulation showing that the common practice of including state-specific time trends will attenuate the measured effects of the minimum wage on employment if the true effect is in fact on the rate of job growth. Using a long state-year panel on the population of private-sector employers in the United States, we find that the minimum wage reduces net job growth, primarily through its effect on job creation by expanding establishments.

Jonathan Meer
Department of Economics
Texas A&M University
College Station, TX 77843
and NBER
jmeer@econmail.tamu.edu

Jeremy West
Department of Economics
Texas A&M University
College Station, TX 77843
j.west@tamu.edu

1 Introduction

The question of how a minimum wage affects employment remains one of the most widely studied – and most controversial – topics in labor economics. During the recent recession, the employment rate for younger or low-skilled workers (who are more likely to be paid wages at or close to the minimum) worsened disproportionately, and following the recession the unemployment gap based on education remains large (Hoynes et al., 2012; United States Bureau of Labor Statistics, 2013). A more conclusive understanding is needed of the effects of the minimum wage if it is to be evaluated alongside alternative policy instruments as a method of increasing the standard of living for low-income households. Moreover, in recent years a number of states have indexed their minimum wages to adjust for inflation, and – despite growing state and federal pressure to continue this practice – there is little evidence on how inflation indexing might alter any effect of the minimum wage.

To date, nearly all studies of the minimum wage and employment have focused on how a legal wage floor affects the employment *level*, either for the entire labor force or a specific employee subgroup (e.g. teenagers or food service workers). We argue that, in a Diamond (1981)-type worker search and matching framework, an effect of the minimum wage should be more apparent in employment *dynamics* – that is, in the actual creation of new jobs by expanding establishments and the destruction of existing jobs by contracting establishments. Diamond argues that transitions to a new employment steady state may be slow, such that it may take some time for any effect of the policy to be visible in the employment level.

In addition to this theoretical foundation, there are several empirical reasons for why effects of the minimum wage should be detected more clearly in job growth than in employment levels. A critical factor is that, unlike many treatments studied in the program evaluation literature, the identifying variation consists of relatively small and temporary changes in a state's real minimum wage, which are soon dissipated by inflation and increases by other states; we confirm this empirically in Section 2.2. As a result, there is often insufficient time for even sizable effects on the rate of job growth to be reflected in the level of employment.

We also demonstrate that a common practice in this literature – the inclusion of state-specific time trends as a control – will attenuate estimates of how the minimum wage affects the employment level. Specifically, we perform a simulation exercise which shows that if the true effect of the minimum wage is indeed in the growth rate of new employment, then even real causal effects on the level of employment will be attenuated and statistically indistinguishable from zero. In contrast, the inclusion of state time trends does not induce a similar attenuation problem for estimates of the direct effect on growth.

To implement our analysis, we use a panel difference-in-differences approach in which we allow for state effects, region-by-year effects, and state-specific time trends. We examine effects of the minimum wage on employment dynamics and levels in the *Business Dynamics Statistics*, a long (1977-2011) panel of administrative data on the aggregate population of private-sector employers in the United States.

Results indicate that job growth declines significantly in response to increases in the minimum wage. However, we do not find a corresponding reduction in the *level* of employment in specifications that include linear time trends. For reasons discussed below and illustrated in our simulation exercise, we view this as neither surprising nor likely to be an accurate reflection of the effect of the minimum wage. Finally, we decompose the net effect on job growth and find that it is primarily driven by a reduction in job creation by expanding establishments, rather than by an increase in job destruction by contracting establishments. These are among the most policy-relevant outcomes related to employment: the change in the number of jobs in the economy, rather than, for instance, the turnover of individuals within existing jobs.

We perform several robustness checks to test the validity of our identification strategy, which requires that the pre-existing time-paths of outcomes for states which increase their minimum wages do not differ in an off-trend manner from those in states that do not see an increase. We evaluate this possibility in Section 5.4 by including leads of the minimum wage into our specifications; if increases in the minimum wage showed an effect on employment dynamics *before* their implementation, this would suggest that the results are being driven by unobserved trends. This is not the case. Indeed, for our results to be driven by confounders, one would have to believe that increases in the minimum wage were systematically correlated with unobserved shocks to that state in the same year, but not other states in that region, unrelated to existing state-specific time trends, and that these shocks are not reflected in measures of state-specific demographics or business cycles. Our results are additionally robust to varying the specifications to account for the recent financial crisis, inflation indexing of state minimum wages, and states that have never implemented a super-federal minimum wage.

The primary implication of our study is that the minimum wage does affect employment through a particular mechanism. This is important for normative analysis in theoretical models (e.g. Lee and Saez, 2012) and for policymakers weighing the tradeoffs between the increased wage for minimum wage earners and the potential reduction in hiring and employment.

This article proceeds as follows. In Section 2 we provide a brief review of the literature on the employment effects of the minimum wage and build our case for examining job growth directly rather than the employment level. Section 3 presents our econometric model and Section 4 describes the *Business Dynamics Statistics* and other data used in our study. We discuss our empirical results in Section 5 and conclude in Section 6.

2 Theoretical and Empirical Framework

2.1 Challenges in Estimating Employment Effects

The economic literature on minimum wages is longstanding and vast. Neumark and Wascher (2008) provide an in-depth review of the field, which continues to be characterized by disagreement on how a minimum wage affects employment. The majority of recent studies, following Card and Krueger (1994), use difference-in-differences comparisons to evaluate the effect of these policies on employment levels. It is important to note that these models test whether there is a discrete change in the level of employment before and after a state changes its minimum wage, relative to the counterfactual change in other state(s)' employment.[1] Two issues arise with this approach: first, the minimum wage is often set at the state level, and different states may not serve as useful counterfactuals; second, the true effect on employment outcomes may not be discrete in levels.

The first issue has received a great deal of attention. A large literature builds upon the basic difference-in-differences framework by modifying the specification to improve the quality of the counterfactuals, and recent empirical specifications often include covariates that capture non-linear differences in local economic climates. For example, Orrenius and Zavodny (2008) use a broad set of business cycle controls (in addition to time period dummies) to account for differing economic environments across states and over time; they find no adverse effects of the minimum wage on the employment of less-educated adults. Taking this a step further, Allegretto et al. (2011) demonstrate substantial heterogeneity in employment patterns across regions of the U.S. and control for this by allowing time period effects

[1] Using within-state variation still leverages changes to the federal minimum wage: if the federal wage floor increases, this effectively acts as a "negative" treatment to the wage differential between states that already use a super-federal minimum wage (and leave it unchanged) and those for whom the federal minimum binds. Baskaya and Rubinstein (2012) employ an interesting hybrid approach. They allow state-driven variation in minimum wage levels to determine the potential impact to each state of a change to the federal minimum wage, and then instrument for these "minimum wage gaps" in a structural model using factors such as political ideology. They find that minimum wages have significant disemployment effects for teenagers, but only when accounting for this variation across states.

to vary by Census Division; they similarly find no effect of the minimum wage on teenage employment. State- or county-specific time trends are often included (e.g. Page et al., 2005; Addison et al., 2009; Allegretto et al., 2011) to control for pre-existing time-paths in how labor markets evolve within different areas. Recently, even more creative approaches have been employed in efforts to improve the counterfactual: Dube et al. (2010) compare contiguous counties across state lines and find no detrimental effect from minimum wage differentials; Sabia et al. (2012) conduct a synthetic control study and find that a state's increased minimum wage had a large and significantly negative employment elasticity for low-wage teenagers and young adults.

These more complex specifications arguably provide for better counterfactual employment variation, but this improvement comes at a cost: increasing the number of econometric controls reduces the amount of variation available to identify an effect. A recent paper by Neumark et al. (2013) is highly critical of some of these approaches, arguing that these studies have "thrown out so much useful and potentially valid identifying information that their estimates are uninformative or invalid." In a parallel vein, we discuss how state time trends in particular affect estimation.

Difference-in-differences identification strategies will only find an effect on the level of employment (at least with short panels) when there is a sufficiently rapid drop in the number of jobs relative to the counterfactual. Given the small margin of net job expansion relative to total employment, illustrated in Figure 1, this effectively necessitates a (temporary) reduction in the absolute size of the labor force. If, instead, the minimum wage affects the rate of net job growth, then it will take some time for the effect to be reflected in the level of total employment to a degree which would be statistically detectable in employment levels specifications.

More broadly, if the true effect of a policy is to change the *slope* for the outcome variable, rather than its *level*, then including time trends as controls will attenuate estimates of the policy's effect; that is, the inclusion of state-specific time trends leads to biased estimation.[2] The basic intuition is that including state-specific time trends as controls will adjust for two sources of variation. First, if there is any *pre-treatment* deviation in outcomes that is correlated with treatment – e.g. if states that exhibit stronger employment growth are also more likely to increase their minimum wage – then this confounding variation may be appropriately controlled for by including state-specific time trends. The potential cost of this added control is that if the actual treatment effect, the *post-treatment* employment variation,

[2]We are grateful to Cheng Cheng and Mark Hoekstra for this insight.

acts upon the trend itself, then inclusion of state time trends will attenuate estimates of the treatment effect and often leads to estimating (statistical) null employment effects.[3]

We illustrate this attenuation problem using both a simple example and a Monte Carlo simulation in Appendix A. The toy example depicts employment in two hypothetical states which exhibit identical employment growth rates prior to period $t = 0$. After period $t = 0$, the employment growth rate in one state falls relative to that in the other state, but there is no discrete change in the level of employment. We then compute the difference-in-differences of state employment, with and without adjustment for state time trends. The computed employment effect is large and negative when state trends are omitted, but shrinks nearly to zero with the inclusion of state time trends. This occurs despite identical pre-treatment employment trends.

In the simulation, we extend this example. Within each Monte Carlo repetition, we simulate a panel of minimum wages and employment in 51 hypothetical states. We impose a treatment effect which relates the minimum wage to employment growth, but not discretely to the level of employment. Then, we use the simulated state-year values to estimate specifications for the effect of the minimum wage on net job growth and on employment, separately with and without including state time trends. In a set of 10,000 Monte Carlo replications, we show that the estimated effect for the minimum wage on job growth is identical to the true effect, regardless of whether state-specific time trends are included as a control. In contrast, although the estimated effect on employment is large and of the correct sign when state linear trends are omitted, the inclusion of state-specific trends into this estimation manages to attenuate the estimated employment effect to (a small and statistically insignificant) zero, despite the true effect being large and negative by construction. As in the simple example, this attenuation occurs despite: (1) a large true effect on job growth by construction; and (2) no systematic correlation of changes to state minimum wages with state effects, year effects, or state time trends (which were all randomly assigned).

If the minimum wage changes the rate of job creation rather than leading to an immediate drop in the number of jobs, then the general lack of significant effects of the minimum

[3] In a large set of models for how the minimum wage affects employment, Sabia (2009) notes that the estimated elasticity is non-negative only when state-specific time trends are included. He offers two explanations for this peculiarity: "First, it may be that the included state-level time-varying controls failed to capture important differences in retail employment trends associated with minimum wage hikes, and that the negative minimum wage effects found in models 1-9 can be explained by state-specific time-varying unmeasured heterogeneity. However, a second explanation is that the inclusion of state linear time trends reduces potentially important identifying variation, thus rendering minimum wage effects small and insignificant." Our discussion above – that the treatment effect is in the trend itself – forms a third explanation.

wage on employment levels in specifications including time trends is not surprising. This is not necessarily an argument against the inclusion of state time trends as controls, however, because of the aforementioned potential for pre-treatment deviations in employment outcomes. Instead, we argue that using net job growth as an outcome provides for a more compelling estimate of the employment effect of the minimum wage. As discussed in the following sub-section, the justification for our approach is two-pronged, motivated both by theoretical arguments and econometric concerns.

2.2 The Case for Examining Employment Dynamics

The basic analysis of the effects of the minimum wage argues for rapid adjustments to a new equilibrium employment level (e.g. Stigler, 1946). However, transitions to a new employment equilibrium may not be smooth (Hamermesh, 1989) or may be relatively slow (Diamond, 1981; Acemoglu, 2001). In this case, the effects of the policy may be more evident in net job creation. The basis for our framework is the role of the minimum wage in a worker search and matching model (e.g. Van den Berg and Ridder, 1998; Acemoglu, 2001; Flinn, 2006, 2011), summarized concisely in Cahuc and Zylberberg (2004). In this class of models, the minimum wage has opposing effects on job creation. Although it reduces demand for labor by raising the marginal cost of employing a new worker, a higher minimum wage increases the gap between the expected returns to employment relative to unemployment, inducing additional search effort from unemployed workers. By increasing the pool of searching workers (and the intensity of their searching), the minimum wage improves the quality of matches between employers and employees, generating surplus. The theory thus has ambiguous predictions for the effect of a minimum wage on job creation. If workers' additional search effort sufficiently improves the worker-firm match quality, then job creation should not be adversely affected and may even increase. However, if the demand-side effect dominates, then increasing the minimum wage will cause declines in hiring.

The effect of the minimum wage on worker separations is ambiguous as well, but there are compelling reasons to expect a smaller effect on job destruction. For employers, the non-trivial fixed cost associated with hiring a new employee (e.g. screening, interviewing, training) likely encourages reductions in hiring rather than increased layoffs (Oi, 1962). Psychological factors may additionally lead to a tempered effect of the minimum wage on job destruction. There is a growing management literature on the negative feelings managers have when terminating employees, sometimes called "firing aversion" (Folger and Skarlicki, 1998; Molinsky and Margolis, 2005; Dubinsky et al., 2011). These studies posit that managers are disinclined to terminate an employee – even if a layoff is justifiable on economic grounds – because "the decision often produces sorrow or guilt, or both" (Gilbert, 2000). Further offsetting any disemployment effect, the minimum wage increases the compensation for a subset of employees; these workers may be less likely to voluntarily exit employment, especially if they possess firm-specific human capital (Hamermesh, 1987).

Furthermore, among workers paid hourly in the United States, additional tenure is highly associated with increased pay. It follows that minimum wage employees are likely to be relatively recent hires, a finding documented by Even and Macpherson (2003) and Dube et al. (2011). A direct implication is that minimum wage increases are most likely to affect workers who are (or would be) recent hires.

To test whether or not this is true empirically, we use the Current Population Survey's Merged Outgoing Rotation Groups (CPS-MORG) to determine the proportion of newly-hired employees who are paid their state's minimum wage.[4] Performing calculations similar to those in Even and Macpherson (2003) – though expanded to include another decade of data – we use the weights provided in the CPS-MORG for 3.56 million individuals between 1979 and 2011 who are observed in both sets of interviews (spaced twelve months apart) in which questions on earnings were asked. We find that among all employees, 3.25 percent earn the minimum wage. But among those who are newly employed (that is, not employed in the first wave of interviews but employed in the second), 11.8 percent earn the minimum wage; fully 30.6 percent of minimum wage workers are recently employed. Thus, minimum wage compensation is three-and-a-half times more prevalent among new workers than in the entire labor force.[5] Moreover, transitions out of the minimum wage are common. Following Even and Macpherson (2003) again, we find that among those who were paid the minimum wage in the first wave, 16.8% leave the labor force and 5.8% become unemployed. Among those who remain employed, 59.3% are paid in excess of the minimum wage in the following year. Those individuals are paid a median amount of $0.90 per hour above that year's minimum

[4] The data are made available by the National Bureau of Economic Research. In our evaluation, we consider an employee to be paid minimum wage only when the employee is paid an amount at or below the state's minimum wage by the hour, treating all salaried workers as being paid above the minimum wage. Although it is possible to impute the hourly wage for non-hourly employees in the CPS, there is reason to be skeptical of such imputed values in this type of analysis (Card, 1992). Inclusion of workers who self-report hourly earnings below the minimum wage into the "percent at the minimum" is common in this literature (e.g. Lang and Kahn, 1998; Pedace and Rohn, 2011).

[5] There is also a non-trivial subset of workers paid only somewhat more than the minimum wage who may be affected by an increase. The median nominal state minimum wage increase in our data is 40 cents; 5.4% of all workers and 18.8% of newly-hired workers are paid within this amount of the minimum wage. It is further possible that an even larger share of workers than this are affected by minimum wages in light of recent research on "last-place aversion" by Kuziemko et al. (2012).

wage, or 23% of their previous wage; the 75th percentile of this figure is $2.45 per hour.

Although there is support for employee hiring to be relatively more affected, the effect of the minimum wage on both job creation and job destruction is ultimately an empirical question. And, because the effects on these gross margins are theoretically ambiguous and potentially opposing, the net employment effect of the minimum wage could take several forms that are not mutually exclusive. First, the minimum wage could affect (positively or negatively) the total employment level. Second, by encouraging a longer duration of worker-firm matches, the minimum wage could reduce turnover of employees within existing jobs. Finally, a minimum wage could change the net *flow* of workers into employment by altering the job growth rate. Any of these outcomes are consistent with the theoretical relationships discussed above, but the bulk of the literature has focused on the first relationship, investigating how a minimum wage affects the employment level.

Several recent studies offer exceptions to the focus on employment levels. Dube et al. (2011) examine the relationship between the minimum wage and employee turnover using the 2001-2008 Quarterly Workforce Indicators (QWI). They focus on teenagers and restaurant employees employed in contiguous counties across state lines, and find that the minimum wage reduced both new hiring and separations despite having little effect on contemporaneous employment levels. Brochu and Green (2012) assess firing, quit, and hiring rates in Canadian survey data. They find that workers hired within the previous six months are less likely to separate from their jobs in the presence of a higher minimum wage, a result driven in part by a reduction in firings; they find no effect on workers with longer tenure. Similar to Dube et al. (2011), they find a reduction in hiring rates but do not estimate the net effect on job growth.[6]

It is important to note that even if it were the case that minimum wages just reduce employee turnover, this outcome might be undesirable. Lazear and Spletzer (2012) argue that employee churn is an important component of the labor market because it indicates reallocation of workers to jobs in which they are more productive. They link declines in employee turnover to reduced economic output. More to the point, the total employment effects of the minimum wage are of primary interest for policy-making. It is uncertain what policy goals are served by increasing the tenure of voluntary employment through labor

[6] Two other recent papers examine the minimum wage and employment dynamics by exploiting institutional features of the minimum wage in Portugal (Portugal and Cardoso, 2006) and Germany (Bachmann et al., 2012). These studies also do not focus on job growth. Hirsch et al. (2011) and Schmitt (2013) focus on other channels of adjustment in response to increases in the minimum wage, such as wage compression, reductions in hours worked, and investments in training.

market regulations. We believe that the relevant outcome is employers' creation of new jobs or destruction of existing jobs – the *net* movement of workers into employment. In light of the issues discussed above, this effect may follow a slow process.

Sorkin (2013) builds a model that formalizes this potentially slow adjustment of labor demand and applies it to minimum wage increases. He argues that "the ability to adjust labor demand is limited in the short run" and that this "provide[s] an explanation for the small employment effects found in the minimum wage literature." Fundamentally, this identification problem stems from the "sawtooth pattern" exhibited in states' real minimum wages. Sorkin argues that "difference-in-difference faces challenges in measuring the treatment effect of interest, which in this case is the effect of a permanent minimum wage increase, whenever there are dynamic responses to the treatment and the treatment itself is time-varying."

Historically, minimum wages have been set in nominal dollars and not adjusted for inflation, so any nominal wage differential between two states will become economically less meaningful over time.[7] Furthermore, sooner or later every state experiences a nominal increase in its minimum wage, either due to a revision to a state law or because the federal minimum wage increases. Unlike the slow erosion of nominal minimum wage gaps brought about by inflation, an increase in the counterfactual's minimum wage may quickly close or even reverse this gap. To put it another way, there is no consistent control group in the long run.

We support this using graphs of monthly-frequency state real minimum wage data in Appendix B. Looking first only within-state, Figure 7 shows that the mean real state minimum wage increase during 1977-2011 was 65 cents (median 69 cents). By the time the same state next increased its real minimum wage, which took 58 months on average, the previous increase in minimum wage had eroded – via inflation – to an average cumulative real *decrease* of 12 cents (median -7 cents, see Figure 8). In fact, Figure 9 shows that the 56 percent of state-year real minimum wage increases that were eventually fully eroded by inflation did so in, on average, fewer than twenty months, and the median time elapsed was only nine months. Turning instead to comparisons within Census Region, the mean *relative* real increase in state minimum wage was 27 cents (median 15 cents, Figure 10). By the time of the next within-state increase, the prior increase had eroded – both via inflation and from other regional neighbors changing their minimum wages – to an average decrease of 1.7

[7]Ten states now use regional CPI measures to index their minimum wages for inflation, but this is a relatively recent practice (Allegretto et al., 2011). The Federal minimum wage increase proposed by President Obama in 2013 included a provision for annual increases based on inflation, but little is known about how inflation indexing may alter the effects of a minimum wage on employment.

cents (median +2 cents, Figure 11). For those 47 percent of state-year increases which fully eroded relative to regional states, this took only 16 months on average (median 10 months, Figure 12).

Granted, there remain numerous state-year increases in the minimum wage that were never fully eroded by inflation or, in a relative sense, by neighboring states later changing their minimum wages. However, this exercise demonstrates that there is a relatively short duration of time during which a state difference-in-differences estimation can identify the effects of the minimum wage on employment levels. This situation would not be problematic if the minimum wage affected employment in an abrupt, discrete manner. But if the minimum wage predominantly affects job creation, then it may take years to observe a statistically significant difference in total employment.[8] Thus, while it is true that any reduction in job growth should be reflected eventually in total employment, the empirical challenges discussed in this section may preclude identifying the net effect of the minimum wage by examining employment levels directly. As a result, even though the employment level is the outcome predominantly considered in the empirical literature on the minimum wage, we focus instead on the job *growth* rate. We implement this approach in the following sections.

3 Empirical Model

3.1 Econometric Specifications

We estimate four reduced-form specifications for the set of employment outcomes at time t. As discussed in Section 1, we begin by assessing the overall rate of net job growth, calculated by the Census Bureau as job creation at time t minus job destruction at time t, divided by the average of employment at times t and $t-1$ (United States Census Bureau, 2012). We argued in Section 2 that this outcome serves as a better measure of the true employment effect of the minimum wage. We additionally estimate the effect on the natural log of employment and contrast these results with those on job growth. Finally, we decompose the effect on net job growth into its respective margins of (log) job creation and (log) job destruction. Our specifications take the following forms:

$$\text{Employment Outcome}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_t + \epsilon_{st} \tag{1}$$

[8] An additional econometric explanation for statistical insignificance is over-saturating a model beyond the power of the data – a non-trivial concern, especially for studies including both linear state time trends and Census Division-specific period fixed effects.

$$\text{Employment Outcome}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_{rt} + \epsilon_{st} \tag{2}$$

$$\text{Employment Outcome}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_{rt} + \psi_s \cdot t \ + \epsilon_{st} \tag{3}$$

$$\text{Employment Outcome}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_{rt} + \psi_s \cdot t \ + \text{Controls}_{st}\Gamma + \epsilon_{st} \tag{4}$$

Specification 1 estimates the classic panel difference-in-differences, examining the impact of the minimum wage after controlling for state (ϕ_s) and year (τ_t) fixed effects. Because different regions of the country may face heterogeneous economic shocks that are correlated with changes in the minimum wage, we adapt Specification 2 to allow the time fixed effects to vary across Census Regions (τ_{rt}). However, these approaches fail to capture variation in state employment *trends* over time that may be caused by factors that are correlated with changes in the minimum wage, so we add state linear time trends ($\psi_s \cdot t$) in Specification 3. To capture residual *non-linear* variation in state economic environments, several socioeconomic controls (discussed in Section 4.3) are added in Specification 4.

As we discuss in detail above, the inclusion of state-specific time trends acts as a double-edged sword: on one hand, this controls for pre-treatment deviations in employment outcomes that may be correlated with changes to a state's minimum wage, but on the other hand, this practice will attenuate estimates of employment effects if the true effect acts on the growth rate rather than the employment level. We argue for examining the effect of the minimum wage on net job growth, with the inclusion of state time trends as a control, an approach supported by our Monte Carlo exercise. Thus, this final specification – specifically, examining the effects on net job growth – is our preferred model, as it most thoroughly accounts for potential confounders while overcoming the potential attenuation problem induced via inclusion of state time trends.

3.2 Identification Concerns

Although we include time trends and other time-varying state characteristics, it remains possible that unobserved systematic off-trend deviations drive the correlation between employment outcomes and changes in the minimum wage. Moreover, because there is often a delay between minimum wage legislation and enactment (see Murphy (2005) for some examples), employers may react in anticipation of a future change to the local minimum wage.

In either of these cases, estimated coefficients for leads of the minimum wage will be economically significant. Results from robustness checks including leads (described in Section 5.4) indicate that this is not the case. These results suggest little employment response in advance of a change. But, to the extent that firms do adapt in anticipation of actual adjustments to the minimum wage, this practice reduces the magnitude of the estimated minimum wage effects.

A related concern is that minimum wage legislation passed during expansionary economic climates may take effect in contractionary periods, resulting in a spurious negative correlation between minimum wages and employment (Reich, 2009; Allegretto et al., 2011). This concern seems unwarranted. In our data from 1977-2011, the average state-level unemployment rate is 5.94% in months during which a state increased its minimum wage, compared to 6.07% in months with no minimum wage increase; the difference is statistically insignificant ($p = 0.12$).[9]

Another issue is employer noncompliance. Predictions vary for the theoretical employment effects from employer noncompliance with minimum wage laws, ranging from higher employment levels than with full compliance (e.g. Ashenfelter and Smith, 1979) to the same employment outcome as with compliance (e.g. Yaniv, 2001). Of course, noncompliance may be part of the response to an increased minimum wage, attenuating the effects of these laws. To the extent that increases in minimum wages are positively correlated with the rate of noncompliance, our results may understate their effect relative to that if compliance was complete. Our aggregate data preclude testing for noncompliance, and available microdata such as the Current Population Survey seem ill-suited to this purpose.[10]

To assess the importance of the functional form of the minimum wage term, we also specified the minimum wage in real levels instead of as a real natural log, addressing concerns raised by Baker et al. (1999). Although the results are robust to this alternate specification, we prefer our natural log specification both because of its elasticity-like interpretation and for consistency with the broader literature.

[9] Baskaya and Rubinstein (2012) demonstrate pro-cyclical timing in state increases to minimum wages for states that typically had a super-federal minimum wage.

[10] For instance, although the CPS-MORG includes data on all hourly wages reported in the CPS from 1979-2011, during this time period the median number of unique surveyed individuals per state-year who reported being paid an amount less than the state's minimum wage is twenty-eight employees, with about ten percent of state-years having five or fewer such respondents. This is simply too small a sample to facilitate reasonable inference about minimum wage noncompliance.

4 Data

4.1 Business Dynamics Statistics

The key data for our study are provided by the U.S. Census Bureau in the *Business Dynamics Statistics*.[11] The BDS covers all non-agriculture private employer businesses in the U.S. that report payroll or income taxes to the IRS. The heart of the BDS is the Census Bureau's internal Business Register, which is sourced from mandatory employer tax filings and augmented using the Economic Census and other data to compile annual linked establishment-level snapshots of employment statistics (on March 12th). The Census Bureau releases the BDS as a state-year panel of employment dynamics, currently covering 1977 to 2011.

The BDS has several advantages over other data that can be used to evaluate the effects of the minimum wage. Most importantly, counts of within-establishment job creation and destruction enable us to examine directly the extensive margins on which minimum wages might impact employment. An establishment is the physical location of an employer, whereas a firm is the legal entity that conducts business. Because a firm that is expanding may have some contracting (or exiting) establishments, and vice versa, summing gross job creation and destruction from the establishment level provides for a more accurate computation of these values (Haltiwanger et al., 2010).

The employer-sourced administrative nature of the BDS brings additional power to our research question. As discussed in Section 2, a higher minimum wage may induce additional searching effort on the part of the currently unemployed. Mincer (1976) shows that this positive supply elasticity often leads to an increase in the number of *unemployed* that differs substantially from the change in employment. Because employment is the policy-relevant outcome, measuring job counts using employer-sourced data provides a better identification of any disemployment effects of the minimum wage than do surveys of individuals, such as the Current Population Survey. Moreover, as demonstrated by Abraham et al. (2009), employment data directly reported by firms to maintain legal compliance are more accurate than responses to individual-level surveys such as the CPS.

To the best of our knowledge, we are the first to estimate effects of the minimum wage using the BDS. However, researchers working on related questions have used administrative employment data in the *Quarterly Workforce Indicators* (e.g. Thompson, 2009; Dube et al., 2011) and the *Quarterly Census of Employment and Wages* (e.g. Addison et al., 2009; Dube

[11] All of the data and code used in this study are available from the authors online and by request. The Census Bureau hosts the BDS at `http://www.census.gov/ces/dataproducts/bds/`.

et al., 2010). Unlike the BDS, which is sourced from IRS payroll tax records, both the QWI and QCEW draw on state unemployment insurance filings. The QWI includes measures of job creation and job destruction directly, while the QCEW does not, making the QCEW unsuitable for our approach.

Compared to the BDS, the main advantage of the QWI is that it offers finer measures of employees' geographic location (county instead of state), industry sector, and aggregate demographics; for our research design, the major shortcoming of the QWI is the substantially shorter length of the panel. The starting date for QWI participation varies considerably across states, but many are relatively recent; thus, Dube et al. (2011) begin their study period in 2001. In light of the issues (especially real minimum wage erosion) discussed in Section 2, a longer span of data is preferable to study the employment effects of the minimum wage. Additionally, the years spanned by the QWI panel exhibit fairly high frequency variation in effective state minimum wages (see Figure 2). Baker et al. (1999) demonstrate that the frequency of changes to state minimum wages within a panel has major implications for estimated employment effects, and that estimates based on shorter panels are especially sensitive to high frequency variation of minimum wages. Spanning 1977 to 2011, the *Business Dynamics Statistics* form the longest available panel of administrative data on U.S. employment dynamics, best ensuring that our estimated reduced-form coefficients reflect the true relationship between the minimum wage and employment outcomes.

Finally, population-level data such as the BDS provide for a cleaner assessment of the overall policy impact of minimum wages by avoiding sampling error and enabling us to obtain more precise estimates.[12] These gains do not come without cost. In particular, we are unable to assess any heterogeneity of labor market effects across demographic groups. Nor can we analyze wage dynamics. We view these costs as unfortunate but worthwhile, justified by the advantages of obtaining a compelling answer to the question of a minimum wage's employment effects.

[12]The BDS still contains nonsampling errors such as typographical errors made by businesses when providing information. The Census Bureau acts to minimize erroneous values, such as excluding from deaths establishments that "exit" the employer universe only to re-enter at some later time. Regardless, as noted by Haltiwanger et al. (2009) nonsampling errors are likely to be distributed randomly throughout the data.

4.2 State Minimum Wages

We sourced historical data on state minimum wages directly from state statutes.[13] We use annual minimum wage values as of March 12th, directly corresponding to the panel years in the BDS data. Some states have used a multiple-track minimum wage system, with a menu of wages that differ within a year across firms of different sizes or industries; we therefore use the maximum of the federal minimum wage and the set of possible state minimum wages for the year. To the extent that there is firm-level heterogeneity in the applicable wage level, our definition allows the minimum wage term to serve as an upper bound for the minimum wage a firm would actually face. We transform minimum wages into constant 2011 dollars using the CPI-U from the Bureau of Labor Statistics.[14]

4.3 Other Control Variables

Although our econometric specifications include an extensive set of time period controls, precision may be gained by accounting for additional state-specific time-varying covariates. We merge the BDS and minimum wage data with state-level controls from several sources.

The Census Bureau's Population Distribution Branch provides annual state-level population counts, including estimates for intercensal values. Total state population represents a determinant of both demand for (indirectly by way of demand for goods and services) and supply of employees. Because states differ non-linearly in their population changes, controlling directly for population may be important. The range in population between states and across time is enormous, so we use the natural log of state population in our specifications. We additionally include the share of this population aged 15-59, which provides a rough weight for how population might affect demand for versus supply of labor. Demographic controls such as these are commonly used in this literature (e.g. Burkhauser et al., 2000; Dube et al., 2010). Following Orrenius and Zavodny (2008), we also include the natural log

[13] Although historical state minimum wage data are available from sources such as the U.S Department of Labor (`http://www.dol.gov/whd/state/stateMinWageHis.htm`), these data suffer several limitations. For one, minimum wage values are only reported as of January first each year, whereas the panel used in our study necessitates values as of March 12th. Additionally these DOL data incompletely characterize changes to state minimum wages, especially during the early years of our panel. This DOL table is frequently used as the source of historical state minimum wage values for recent studies in this literature, and we caution future researchers to be careful not to inadvertently attribute minimum wage changes to years in which they did not occur. The full set of data we use is available by request from the authors and provided online.

[14] Because we use a national-level deflator, specifying the log minimum wage term as real versus nominal does not affect our results. Time period fixed effects incorporate this added variation. Our findings are similarly unchanged when using a minimum wage that is weighted by the number of days during each panel year that the value was in effect, rather than the static value on March 12th each year.

of real gross state product per capita.[15] After controlling for state population, this term can be thought of as a rough proxy for average employee productivity as well as a measure of state-level fluctuations in business cycles (Carlino and Voith, 1992; Orrenius and Zavodny, 2008).

Our study uses data on the fifty states and Washington, D.C. from 1977-2011, but drops observations for Alaska in 1989 and 1990 and Oregon for 1993 and 1994 because of a data quality issue, leaving 1781 state-year observations included in our specifications.[16] We present summary statistics for state minimum wages and employment variables in Table 1. As discussed in Section 2.2, a more detailed presentation of state minimum wage levels and changes is available in Appendix B.

5 Results

5.1 Net Job Growth

We begin by examining the effects of the minimum wage on the net job growth rate in Row [1] of Table 2. Column (1) of Row [1], corresponding to Specification 1 above, shows a negative and statistically significant effect for job growth, which is effectively unchanged by switching to region-by-year effects in Column (2). Adding state time trends in Column (3) makes the coefficient somewhat larger. Column (4), which includes state-specific time trends and fixed effects, additional controls, and region-by-year effects, yields a coefficient of -0.053, with a state-clustered standard error of 0.017. Because the outcome is defined as a growth rate, the result in Column (4) indicates that a real minimum wage increase of ten percent reduces job growth in the state by around 0.53 percentage points (during these years, the average state employment growth rate was 2.0 percent annually). In other words, a ten percent increase to the minimum wage results in a reduction of approximately one-quarter of the net job growth rate.

Recall also that we are examining the entire labor market. To the extent that not all workers are affected by increases in the minimum wage, the effect is likely to be more

[15] We compute the log of the real value of total GSP per capita using all industry codes, including government. Results are virtually unaffected by using ln(real *private sector* GSP/capita) instead, but we view total GSP as the more appropriate definition given that the population term reflects total state population.

[16] For instance, the annual change in employment is about 35 percent from 1993 to 1994 in Oregon, an implausibly large magnitude that dwarfs any annual change seen in the data, including those during the recent recession. Discussions with authorities in the Census Bureau revealed that data corruption necessitated coarsely imputing values. We elected to drop these four observations, none of which were associated with a change in the nominal minimum wage.

concentrated on these portions of the wage distribution. Therefore, this result implies a large reduction in the rate of net new positions created, one that may appear implausible on first inspection.

This effect cannot be directly compared to estimates of the elasticity of employment with respect to the minimum wage; in particular, there is a temptation to extrapolate this effect by exponentiating the lower growth rate over a long period. This extrapolation assumes a policy in which a state *permanently* raises its real minimum wage by a constant amount over its counterfactual. As discussed in Section 2, there are frequent changes and thus no long-term comparison group. For this reason, calculations that extend this change in the growth rate for many years are strongly out-of-sample and therefore unreliable. As a more direct comparison, we note that this decrease in the net job growth rate implies an elasticity of -0.05 in total employment after one year.[17] Again, though, this effect will be proportionally larger for the portion of the workforce that is sensitive to the increase. This hypothetical increase will continue to have an effect in future years, though as discussed in Section 2, it will be eroded both by inflation and by the changes in the state's comparison group. The effective elasticity over the typical relevant time frame is -0.1204.[18] That is, each ten percent increase in a state's real minimum wage, relative to its regional neighbors, causes a 1.2 percent reduction in total employment relative to the counterfactual by the end of five years.

5.2 Total Employment

We directly examine the elasticity of employment with respect to the minimum wage in Row [2] of Table 2.[19] The estimated elasticity is large and negative in Columns (1) and (2), which do not include differential state trends. However, the specifications in Columns (3) and (4), which do include these trends, yield small and statistically insignificant effects. Thus, we can replicate a common – though certainly not universal – result in the literature of no measured

[17] A reduction in the growth rate by 0.53 percentage points, evaluated at the mean, results in approximately 10,000 fewer jobs relative to the counterfactual in the following year on a baseline of approximately 1.9 million jobs, yielding an elasticity of -0.05.

[18] Suppose that in year one a state increases its real minimum wage by 10%, relative to other states within its Census region. The average erosion rate in our panel predicts a remaining effective difference of 6.62% in year two. This relative difference shrinks to 4.25% by year three, to 2.43% by year four, and to 0.77% by year five, before fully eroding. This suggests a cumulative five-year effect that is 2.41 times that observed in year one.

[19] Note that the elasticities we estimate for employment are for *total* employment, reflecting virtually the population of private-sector jobs in the U.S., so a direct comparison may be inappropriate with much of the literature, which often focuses on employment for some specific employee subgroup.

effect of the minimum wage on the level of employment; this is unsurprising in light of our discussion in Section 2.

Recall that the results in Columns (3) and (4) identify changes relative to each state's linear time trend. Essentially, then, our result is that of no *discrete* change in employment levels relative to trend. This is consistent with theories of relatively slow transitions to a new employment steady state, as in Diamond (1981) and Acemoglu (2001), as well as our discussion of the econometric issues involved in estimating the effects on employment levels in Section 2. Likewise, there is strong evidence of an attenuation problem induced by the state time trend controls. The estimated employment elasticity is large and negative in specifications (1) and (2), but the coefficient moves close to zero when state time trends are included in columns (3) and (4). In contrast, point estimates for the effect on the job growth rate in Row [1] remain fairly similar across specifications. As we discussed in Section 2, there could be *pre-treatment* variation in employment outcomes that is correlated with changes to state minimum wages, so we do not advocate the exclusion of state time trends as controls. Rather, we argue that the results for net job growth serve as a more compelling estimate of the true disemployment effect of the minimum wage.

5.3 Job Creation by Expanding Firms and Job Destruction by Contracting Firms

Although the results for net job growth provide an overall measure of the policy's effect, it may be helpful to better understand how the minimum wage effects each component piece of net job growth. In this section, we separately estimate the gross effects of the minimum wage, beginning with the creation of jobs by expanding establishments. Column (4) of Row [3] shows an estimated elasticity of -0.198, with a standard error of 0.058. That is, a ten percent increase in the minimum wage reduces the gross creation of new jobs in expanding firms by about 2.0 percent. Evaluated at the mean, this implies that approximately 6,200 fewer total new jobs are created each year in response to a state minimum wage increase of ten percent. Using the same time frame measure as in Section 5.1, this results in an cumulative reduction in the creation of new jobs by about 15,000 jobs.

The effect of the minimum wage on job destruction, in Row [4], is somewhat less clear. The point estimate is statistically significant only in Column (4) and only at the 10% level; it is also not robust to the tests in Section 5.4. Furthermore, because we cannot say whether these reductions are the result of establishments choosing not to fill vacancies created by voluntary separations, or a proactive decision to engage in labor force reduction, we do not make very much of this result.[20] We therefore conclude that the changes in the net job growth rate are primarily due to the decrease in job creation by expanding establishments, rather than an increase in job destruction by contracting establishments.

5.4 Robustness Checks

In this section, we present a number of alternative specifications to assess the robustness of our results. In particular, we perform a common falsification test, as well as demonstrating invariance of our results to accounting for minimum wage inflation indexing, dropping the years of the recent financial crisis, and using a counterfactual including only states that ever implemented a super-federal minimum wage.[21]

5.4.1 Leading Values

If increases in the minimum wage appear to have an effect on employment dynamics *before* their implementation – especially if contemporaneous changes lose their effect – then our results might be driven by unobserved trends. To investigate this possibility, we include leading values of the minimum wage as a check of pre-existing deviations in the trend between treated states and their counterfactual. Table 3 presents these results for job creation and destruction, employment, and the job growth rate. Column (1) reproduces the baseline results from Column (4) of Table 2 as a reference. In Column (2), we see that including two leads leaves the main results effectively unchanged (the coefficient for net job growth is significant at $p = = 0.06$). The sole exception are the results for job destruction, as described above, for which the main coefficient becomes small and insignificant. None of the leads are statistically significant and most are quite small.[22] The leading terms indicate no deviation in the trend of employment outcomes prior to the actual change. This is a reassuring confirmation that our results are not being driven by unobserved trends.

[20] We also examine establishment entry and exits, in part motivated by Burdett and Mortensen (1998) and Van den Berg and Ridder (1998), who posit that increasing a minimum wage may eliminate firms which are unprofitable at the higher wage level. We find no evidence of effects on these outcomes.

[21] As an additional check on the robustness of our specifications, we separately include either quadratic state time trends or Census *Division* by time period effects (there are four Census Regions containing nine Divisions). Results from either of these specifications (available upon request) are similar in sign and magnitude to those reported.

[22] Results when including only a single leading value are nearly identical to those presented for two leads.

5.4.2 Inflation-Indexing

Next, in order to assess whether states that have shifted to indexing their minimum wage for inflation affect our results, we drop these observations. Using data on wage indexing from Allegretto et al. (2011), this leaves 1726 observations. The results, found in Column (3) of Table 3, are very similar to our main results. It may not be surprising that automatic increases are not driving our results – not necessarily because they are more predictable, but because these policies are relatively recent and affect few observations.

5.4.3 Financial Crisis

The 2008-2009 recession saw striking changes in employment. Because we include time period fixed effects (often by region), the recent recession should not unduly affect our results. However, these two years of our panel additionally experienced several large and high frequency changes in real minimum wage levels, primarily resulting from the federal increases during these years (see Figure 2). As a check that these particular years are not overly influencing identification of the minimum wage term, we estimate specifications using only pre-2008 data (including 1577 observations). As seen in Column (4) of Table 3, the coefficients are not meaningfully different from our main results.

5.4.4 States With No Super-Federal Minimum Wage

During our sample period, seventeen states never required firms to pay a minimum wage greater than the contemporaneous federal wage floor. In case there is something fundamentally different in trend for these states that reduces their effectiveness as a counterfactual in our difference-in-differences estimation, we assessed robustness of the results to dropping these states. The results using only the thirty-four states which implemented a super-federal minimum wage at some point during our panel (1186 observations) are in Column (5) of Table 3. They are similar to those in our primary results; we do not believe that the other seventeen states make for a worse counterfactual – as plots of employment trends confirm – but it is reassuring to see such a small change in the results and their significance despite dropping nearly a third of the observations.

6 Conclusion

We examine how a wage floor impacts employment by directly assessing employment dynamics. In a worker search and matching model (e.g. Acemoglu, 2001; Flinn, 2011), a minimum wage has two opposing effects on employment: it reduces demand for new workers by raising the marginal cost of an employee, while inducing additional search effort from unemployed workers, potentially improving the employee-employer match quality. The theory shapes our understanding of how a minimum wage affects employment, but the equilibrium result is an empirical question.

We provide both theoretical and empirical reasons to believe that an effect of the minimum wage should be most pronounced on new job growth. In addition, we conduct a simulation showing that the common practice of including state-specific time trends will attenuate the measured effects of the minimum wage on employment if the true effect is in fact on the rate of job growth. In light of these factors, we focus on net job growth and leverage a long panel of aggregate data on the population of private-sector employers in the United States. We find that the minimum wage reduces net job growth, primarily through its effect on job creation by expanding establishments.

The results for job creation show that, in equilibrium, any supply-side effects on search (and the potential increase in the quality of employer-employee matches) do not overcome the negative demand-side effects of higher labor costs. The lack of strong effects on job destruction is in line with the literature on the fixed costs of labor and firing aversion. More importantly, we find that on net the minimum wage meaningfully affects employment via a reduction in the rate of long run job growth.

Our results have implications for the recent proposals to index the minimum wage to inflation. We show that the effects on employment are limited by the erosion due to inflation. Permanent real increases in the minimum wage are likely to have greater impacts than the nominal changes we study.

Following the recent recession, unemployment remains disproportionately high for less educated and inexperienced workers (United States Bureau of Labor Statistics, 2013). In the short run, the economic incidence of increases to the minimum wage primarily falls on employers, as evidenced by the lack of significant layoffs. In the long run, this group of workers faces substantially longer periods of unemployment or delays in hiring, thus bearing more of the cost from minimum wages. This phenomenon is particularly important given the evidence that minimum wage jobs often result in relatively rapid transitions to higher-paying jobs.

References

Abraham, K. G., Haltiwanger, J. C., Sandusky, K., and Spletzer, J. (2009). Exploring differences in employment between household and establishment data. Working Paper 14805, National Bureau of Economic Research.

Acemoglu, D. (2001). Good jobs versus bad jobs. *Journal of Labor Economics*, 19(1):1–20.

Addison, J. T., Blackburn, M. L., and Cotti, C. D. (2009). Do minimum wages raise employment? Evidence from the U.S. retail-trade sector. *Labour Economics*, 16(4):397–408.

Allegretto, S. A., Dube, A., and Reich, M. (2011). Do minimum wages really reduce teen employment? Accounting for heterogeneity and selectivity in state panel data. *Industrial Relations*, 50(2):205–240.

Ashenfelter, O. and Smith, R. S. (1979). Compliance with the minimum wage law. *Journal of Political Economy*, 87(2):333–350.

Bachmann, R., König, M., and Schaffner, S. (2012). Lost in transition? Minimum wage effects on German construction workers. Ruhr Economic Papers 358.

Baker, M., Benjamin, D., and Stanger, S. (1999). The highs and lows of the minimum wage effect: A time-series cross-section study of the Canadian law. *Journal of Labor Economics*, 17(2):318–350.

Baskaya, Y. S. and Rubinstein, Y. (2012). Using federal minimum wages to identify the impact of minimum wages on employment and earnings across the U.S. states. Working paper, The Society of Labor Economists.

Brochu, P. and Green, D. A. (2012). The impact of minimum wages on quit, layoff and hiring rates. Working paper.

Burdett, K. and Mortensen, D. T. (1998). Wage differentials, employer size, and unemployment. *International Economic Review*, 39(2):257–273.

Burkhauser, R. V., Couch, K. A., and Wittenburg, D. C. (2000). Who minimum wage increases bite: An analysis using monthly data from the SIPP and the CPS. *Southern Economic Journal*, 67(1):16–40.

Cahuc, P. and Zylberberg, A. (2004). *Labor Economics*. Cambridge: The MIT Press.

Card, D. (1992). Using regional variation in wages to measure the effects of the federal minimum wage. *Industrial and Labor Relations Review*, 46(1):22–37.

Card, D. and Krueger, A. B. (1994). Minimum wages and employment: A case study of the fast-food industry in New Jersey and Pennsylvania. *The American Economic Review*, 84(4):772–793.

Carlino, G. A. and Voith, R. (1992). Accounting for differences in aggregate state productivity. *Regional Science and Urban Economics*, 22(4):597–617.

Diamond, P. A. (1981). Mobility costs, frictional unemployment, and efficiency. *Journal of Political Economy*, 89(4):798–812.

Dube, A., Lester, T. W., and Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics*, 92(4):945–964.

Dube, A., Lester, T. W., and Reich, M. (2011). Do frictions matter in the labor market? Accessions, separations and minimum wage effects. Working paper 5811, IZA.

Dubinsky, A. J., Kim, J., and Lee, S. (2011). Imparting negative news to salespeople. *Psychology and Marketing*, 28(8):803–824.

Even, W. E. and Macpherson, D. A. (2003). The wage and employment dynamics of minimum wage workers. *Southern Economic Journal*, 69(3):676–690.

Flinn, C. J. (2006). Minimum wage effects on labor market outcomes under search, matching, and endogenous contact rates. *Econometrica*, 74(4):1013–1062.

Flinn, C. J. (2011). *The Minimum Wage and Labor Market Outcomes.* Cambridge: The MIT Press.

Folger, R. and Skarlicki, D. P. (1998). When tough times make tough bosses: Managerial distancing as a function of layoff blame. *The Academy of Management Journal*, 41(1):79–87.

Gilbert, J. T. (2000). Sorrow and guilt: An ethical analysis of layoffs. *SAM Advanced Management Journal*, 65(2).

Haltiwanger, J., Jarmin, R. S., and Miranda, J. (2009). Business Dynamics Statistics: An overview. Kauffman Foundation.

Haltiwanger, J., Jarmin, R. S., and Miranda, J. (2010). Who creates jobs? Small vs. large vs. young? Working Paper 16300, National Bureau of Economic Research.

Hamermesh, D. S. (1987). The costs of worker displacement. *The Quarterly Journal of Economics*, 102(1):51–76.

Hamermesh, D. S. (1989). Labor demand and the structure of adjustment costs. *The American Economic Review*, 79(4):674–689.

Hirsch, B. T., Kaufman, B. E., and Zelenska, T. (2011). Minimum wage channels of adjustment. Working paper 6132, IZA.

Hoynes, H., Miller, D. L., and Schaller, J. (2012). Who suffers during recessions? *Journal of Economic Perspectives*, 26(3):27–48.

Kuziemko, I., Buell, R. W., Reich, T., and Norton, M. I. (2012). Last-place aversion: Evidence and redistributive implications. Working Paper 17234, National Bureau of Economic Research.

Lang, K. and Kahn, S. (1998). The effect of minimum-wage laws on the distribution of employment: theory and evidence. *Journal of Public Economics*, 69:67–89.

Lazear, E. P. and Spletzer, J. R. (2012). Hiring, churn and the business cycle. Working Paper 17910, National Bureau of Economic Research.

Lee, D. and Saez, E. (2012). Optimal minimum wage policy in competitive labor markets. *Journal of Public Economics*, 96:739–749.

Mincer, J. (1976). Unemployment effects of minimum wages. *Journal of Political Economy*, 84(4):87–104.

Molinsky, A. and Margolis, J. (2005). Necessary evils and interpersonal sensitivity in organizations. *The Academy of Management Review*, 30:245–268.

Murphy, K. (2005). States give minimum wage earners a boost. *Stateline*.

Neumark, D., Salas, J. I., and Wascher, W. (2013). Revisiting the minimum wage-employment debate: Throwing out the baby with the bathwater? Working Paper 18681, National Bureau of Economic Research.

Neumark, D. and Wascher, W. (2008). *Minimum Wages*. Cambridge: The MIT Press.

Oi, W. Y. (1962). Labor as a quasi-fixed factor. *Journal of Political Economy*, 70(6):538–555.

Orrenius, P. M. and Zavodny, M. (2008). The effect of minimum wages on immigrants' employment and earnings. *Industrial and Labor Relations Review*, 61(4):544–563.

Page, M. E., Spetz, J., and Millar, J. (2005). Does the minimum wage affect welfare caseloads? *Journal of Policy Analysis and Management*, 24(2):273–295.

Pedace, R. and Rohn, S. (2011). The impact of minimum wages on unemployment duration: Estimating the effects using the Displaced Worker Survey. *Industrial Relations*, 50:57–75.

Portugal, P. and Cardoso, A. R. (2006). Disentangling the minimum wage puzzle: An analysis of worker accessions and separations. *Journal of the European Economic Association*, 4(5):988–1013.

Reich, M. (2009). Minimum wages: Politics and economics. In Brown, C., Eichengreen, B., and Reich, M., editors, *Labor in the Era of Globalization*, pages 353–374. Cambridge: Cambridge University Press.

Sabia, J. J. (2009). The effects of minimum wage increases on retail employment and hours: New evidence from monthly CPS data. *Journal of Labor Research*, 30(1):75–97.

Sabia, J. J., Burkhauser, R. V., and Hansen, B. (2012). Are the effects of minimum wage increases always small? New evidence from a case study of New York state. *Industrial and Labor Relations Review*, 65(2).

Schmitt, J. (2013). Why does the minimum wage have no discernible effect on employment? Working paper, Center for Economic and Policy Research.

Sorkin, I. (2013). Minimum wages and the dynamics of labor demand. Working paper.

Stigler, G. J. (1946). The economics of minimum wage legislation. *The American Economic Review*, 36(3):358–365.

Thompson, J. P. (2009). Using local labor market data to re-examine the employment effects of the minimum wage. *Industrial and Labor Relations Review*, 62(3):343–366.

United States Bureau of Labor Statistics (2013). The employment situation: May 2013. BLS monthly news release.

United States Census Bureau (2012). Business Dynamics Statistics variable codebook. http://www.census.gov/ces/dataproducts/bds/data.html.

Van den Berg, G. J. and Ridder, G. (1998). An empirical equilibrium search model of the labor market. *Econometrica*, 66(5):1183–1221.

Yaniv, G. (2001). Minimum wage noncompliance and the employment decision. *Journal of Labor Economics*, 19(3):596–603.

7 Figures and Tables

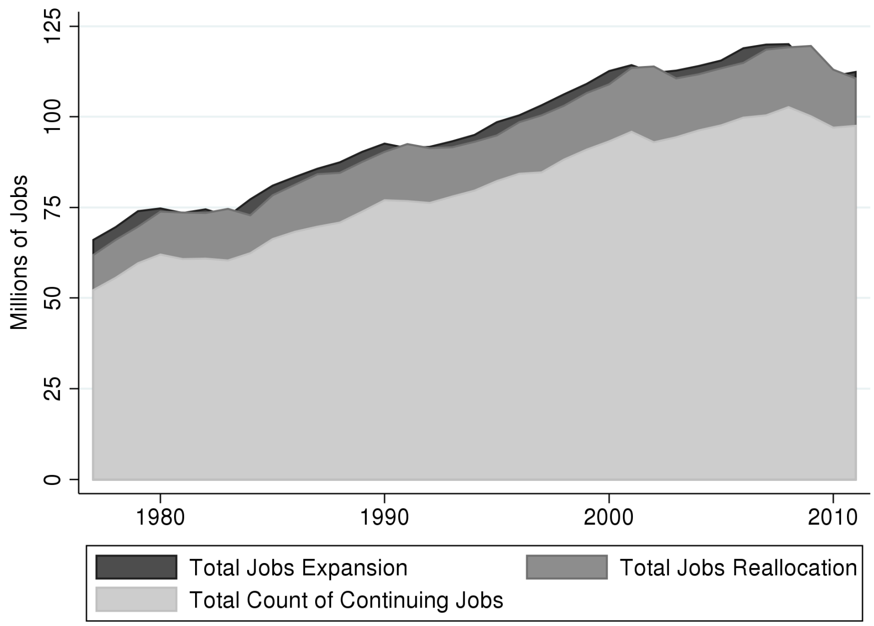


Figure 1: Composition of national job counts across establishments

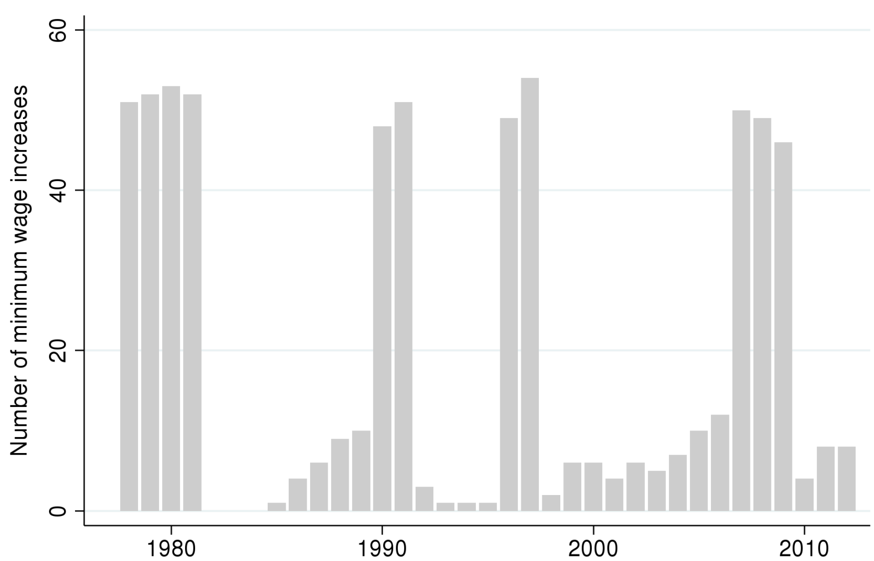


Figure 2: Frequency of increases to effective state real minimum wages

Table 1: Summary statistics

	Mean	Std. Dev.	Median
State minimum wage ($nominal)	4.40	1.360	4.25
State minimum wage ($real 2011)	6.88	0.831	6.73
Employment variables:			
Job growth rate	0.0200	0.0343	0.0210
Jobs (thousands)	1890.4	2104.3	1228.0
Job creation (thousands)	314.3	369.6	206.1
Job destruction (thousands)	282.6	337.6	181.1
State covariates:			
Population (thousands)	5169.1	5732.2	3502.9
Share aged 15-59	0.617	0.0193	0.617
Real GSP/capita (thousands)	41.91	16.16	38.72
State-year observations	1781		

Notes: Employment statistics are computed for the aggregate population of employees in each of the fifty states and Washington, D.C. as of March 12, 1977-2011 using the U.S. Census Bureau's *Business Dynamics Statistics*. We define each state's minimum wage annually on March 12 as the maximum of the federal minimum wage and the state's minimum wage using data from state statutes. All real dollar amounts are indexed to $2011 using the CPI-Urban.

Table 2: Effect of the minimum wage on employment outcomes

	(1)	(2)	(3)	(4)
[1] Job growth rate	-0.0376**	-0.0393**	-0.0500**	-0.0530**
	(0.009)	(0.013)	(0.015)	(0.017)
[2] Log of employment	-0.1790	-0.2374**	-0.0307	-0.0059
	(0.118)	(0.099)	(0.030)	(0.016)
[3] Log of job creation	-0.3376**	-0.3539**	-0.2247**	-0.1981**
	(0.107)	(0.106)	(0.061)	(0.058)
[4] Log of job destruction	-0.0993	-0.1023	0.0783	0.1213
	(0.107)	(0.143)	(0.068)	(0.063)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y			
Census region $*$ year FE		Y	Y	Y
State linear trends			Y	Y
Other controls				Y
State-year observations	1781	1781	1781	1781

** $p < 0.05$ Notes: Each coefficient represents a separate regression of the dependent variable on the natural log of a state's real minimum wage. Robust standard errors are clustered by state and reported in parentheses. Regressions include observations for the aggregate population of employers at the state-year level as of March 12, 1977-2011, as reported in the U.S. Census Bureau's *Business Dynamics Statistics*. The job growth rate is defined annually by the Census Bureau as job creation at time t minus job destruction at time t, divided by the average of employment at times t and $t-1$. "Other controls" consist of the natural log of state population, the share of state population aged 15-59, and the natural log of real gross state product per capita. All dollar amounts are indexed to \$2011 using the CPI-Urban.

Table 3: Robustness checks for the effect of the minimum wage on employment outcomes

	(1) Baseline results	(2) Leading values	(3) Inflation indexing	(4) Financial crisis	(5) Superfederal states
[1] Job growth rate					
Log of minimum wage	-0.0530**	-0.0413	-0.0559**	-0.0630**	-0.0489**
	(0.017)	(0.022)	(0.019)	(0.027)	(0.019)
One year lead		-0.0168			
		(0.024)			
Two year lead		-0.0034			
		(0.015)			
[2] Log of employment					
Log of minimum wage	-0.0059	-0.0158	0.0038	0.0080	0.0008
	(0.016)	(0.018)	(0.018)	(0.020)	(0.015)
One year lead		0.0085			
		(0.013)			
Two year lead		0.0397			
		(0.031)			
[3] Log of job creation					
Log of minimum wage	-0.1981**	-0.2059**	-0.1764**	-0.1824**	-0.1796**
	(0.058)	(0.071)	(0.058)	(0.071)	(0.065)
One year lead		0.0012			
		(0.063)			
Two year lead		0.0138			
		(0.056)			
[4] Log of job destruction					
Log of minimum wage	0.1213	0.0230	0.1685**	0.1743	0.1201
	(0.063)	(0.086)	(0.074)	(0.117)	(0.069)
One year lead		0.1093			
		(0.113)			
Two year lead		0.0399			
		(0.069)			
Observations	1781	1675	1726	1577	1186

** $p < 0.05$ Notes: Column (1) replicates specification (4) from Table 2. Separately: Column (2) adds leads of the log minimum wage; Column (3) drops the 55 observations with an inflation-indexed state minimum wage; Column (4) uses only pre-2008 data; and Column (5) includes only the 34 states which ever had a super-federal minimum wage.

A Attenuated Estimates for Employment Effects

We begin with a very simple example to illustrate how – if a policy's true effect is in a state's growth rate – including state time trends as controls yields biased and misleading difference-in-differences results for the outcome in levels. Then, we conduct a more formal Monte Carlo simulation exercise that underscores this attenuation problem and shows how re-defining the outcome as a growth rate offers a compelling solution. Throughout this appendix, we selected numerical values that as closely as possible match the statistical properties of the actual U.S. employment data used in our study.

A.1 Example of a Disemployment Effect in the Growth Rate

Here, we provide a simple illustration of how including state time trends as controls can sharply attenuate difference-in-differences results, misleading inference. We construct eleven periods (-5 through 5) for two hypothetical states, one of which is treated following period zero. For simplicity, we set the baseline log-employment to zero for the control state and 0.03 for the treated state. During the "pre" periods (through zero), neither state is treated and both states have an employment growth rate of 0.02 log-points per period. During the "post" periods (one through five) the control state maintains this same growth rate, but growth in the treated state drops to 0.015 log-points. Figure 3 illustrates these time paths for the treated state with a solid black line and the control state in grey. The time trend for the treated state is also plotted by a dashed black line. By construction, the control state's time trend perfectly coincides with its log-employment level.

In Figure 4, we compare the difference-in-differences of outcomes between these hypothetical states, without versus with state linear time trends. Panel (a) graphically presents the canonical difference-in-differences model. As is immediately evident in Figure 3, the difference between the states throughout the pre-treatment period is constant at 0.03 log-points. Following treatment, employment between states steadily converges, and the average difference during the post-treatment period is 0.015 log-points. The simple difference-in-differences treatment effect for employment is thus the vertical difference between the two dashed lines: 0.015 minus 0.03, or 50% of the initial difference in state employment.

Panel (b) of Figure 4, rather than showing differences in levels, plots the differences in residuals about each state's time trend. For the control state, which exhibits perfectly linear employment growth, the residuals about trend are always zero, so the heights of the bars mirror residuals in the treated state. As in Panel (a), the dashed lines show the average difference for the pre- and post-treatment periods. In sharp contrast to the average differences in levels shown Panel (a), the average difference in residuals-to-trend is negligibly small in both periods. On average, the difference is 0.00057 pre-treatment and -0.00068 post-treatment. The difference-in-differences is -0.00125 log-points, roughly eight percent of the magnitude in Panel (a), or 4% of the initial difference in state employment.

The inclusion of state time trends in this toy example leads to strikingly misleading inference for the disemployment effect. Moreover, the pre-treatment time paths for the two states are identical in trend by construction, so including state time trends in this model cannot correct for any confounding selection into treatment. In real data, such similarity in pre-treatment trends is not always the case, as we discuss in Section 2, such that omitting state time trends is not a universally advisable solution. Instead, we argue for examining growth rates directly. This approach is supported by the Monte Carlo simulation below.

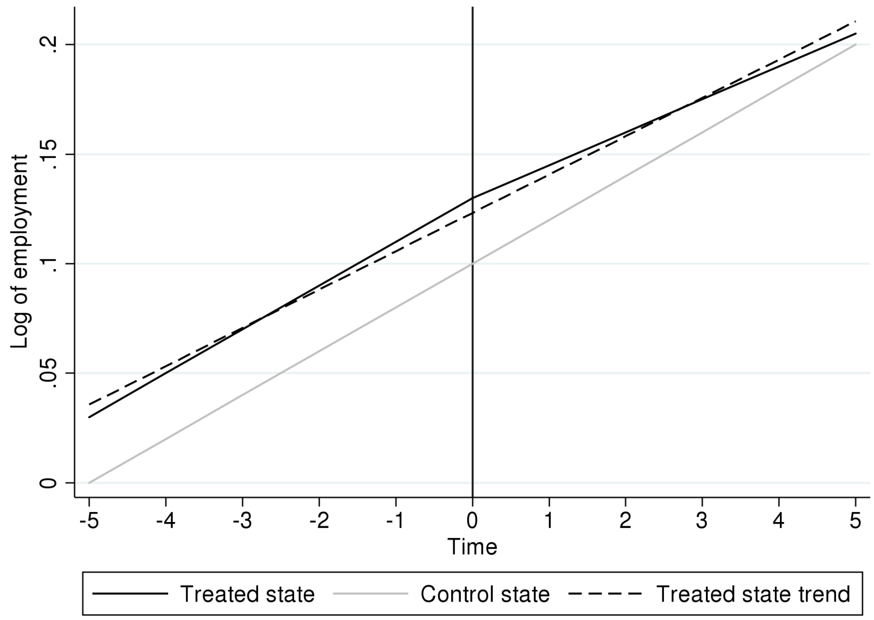
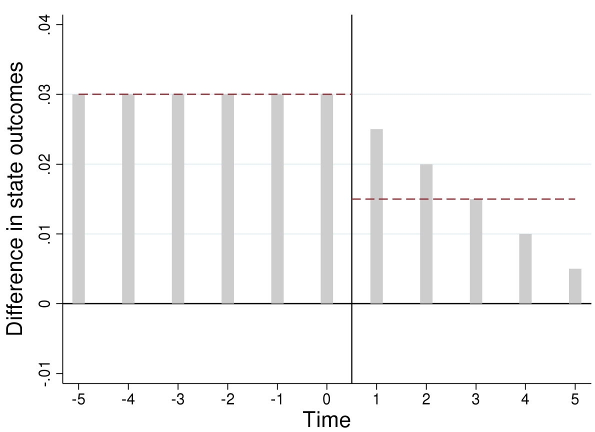
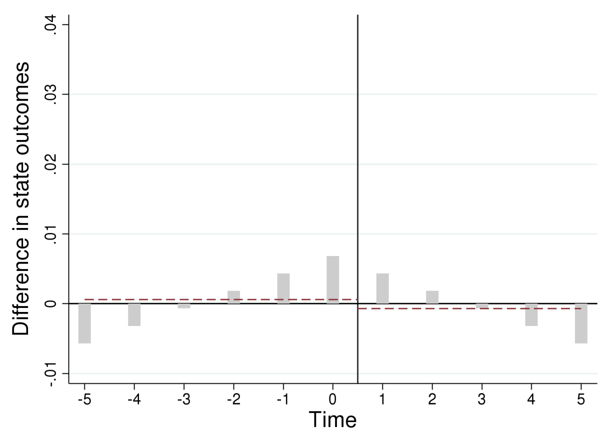


Figure 3: Simple example of disemployment effect in growth rate



(a) Without state trends



(b) Residual to state trends

Figure 4: Difference-in-differences without versus with state linear trends

A.2 Monte Carlo Simulation Exercise

This simulation further illustrates a particular type of attenuation problem for estimates of the employment effect of the minimum wage. The attenuation occurs when the following three conditions hold: (1) the true effect of the policy is on the growth rate of employment, as depicted in the previous example; (2) the employment (log) level is used as the regression outcome; and, (3) the econometric specification includes state time trends as controls. Support for the first condition is discussed at length in Section 2 of the article. The latter two conditions are fairly common in the literature. Section 2 of the article and the previous sub-section of this appendix additionally include discussion of *how* state time trends induce attenuation. Here, we focus on the mechanics of this simulation exercise.

A.2.1 Data Generating Process

We use the actual distribution of changes in state minimum wages and employment as a starting point for our simulated data. Specifically, we compute the first difference in each year for each state's real log minimum wage and log employment. We include values regardless of whether there was a nominal increase in the state's minimum wage, so the sign of many changes in minimum wage is negative. This yields 34 periods of observations on 51 state entities, or 1734 total observed changes in log-employment and minimum wages. Next, we strip these data values of their state and year identifiers and unlink changes in minimum wages from their respective changes in employment. This leaves us with two independent distributions of changes, one for real log minimum wages and one for log-employment; each has 1734 data values.

A.2.2 Steps in Each Monte Carlo Repetition

Within each Monte Carlo repetition, we perform the following steps. First, we draw values without replacement from the distributions of first differences in employment and minimum wages. We assign these values randomly to 34 periods of 51 states, thereby forming a simulated panel of state-year data. $\Delta\text{ln}(\text{employment})_{st}$ and $\Delta\text{ln}(\text{MW})_{st}$ denote the first differences in (log) employment and real minimum wage, respectively, in state s in period t, relative to the previous period (t-1).

Next, we impose a treatment effect relating the minimum wage to the growth rate of employment. To prevent the effect from being purely deterministic, we draw a parameter ϕ_{st} from a $Normal(-0.05, 0.02)$ distribution for each state-year observation. That is, each 10% increase in a state's real minimum wage causes, in expectation, a 0.5 percentage point reduction in employment growth.

Because the effect is on the employment growth rate, the treatment effect in a state in one year persists throughout all future years, although it may be eroded by treatment effects associated with future real decreases in that state's minimum wage. Letting α_{st} denote the treatment effect in a given state-year, this is:

$$\alpha_{st} = \sum_{r=1}^{t} \phi_{sr} \cdot \Delta\ln(\text{MW})_{sr} \qquad\qquad \phi_{sr} \sim N(-0.05, 0.02) \qquad\qquad \forall t \in [1, 34]$$

For each state-year, we then add α_{st} to the previously assigned first difference in employment, forming a new growth pattern which encompasses these treatment effects. Although we include state fixed effects in each specification below, we use the actual employment level for each state in 1977 to set $\ln(\text{employment})_{s0}$ for each of the 51 simulated states. We set $\ln(\text{MW})_{s0}$ for all states equal to zero. Formally:

$$\ln(\text{employment})_{st} = \ln(\text{employment})_{st-1} + \Delta\ln(\text{employment})_{st} + \alpha_{st}$$

$$\ln(\text{MW})_{st} = \ln(\text{MW})_{st-1} + \Delta\ln(\text{MW})_{st}$$

Using these equations, we simulate a panel of log-employment and minimum wages in 51 states in 34 periods, which encompasses the stipulated treatment effect on employment growth. We use this panel to estimate four specifications relating the minimum wage to employment outcomes:

$$\text{employment growth}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_t + \epsilon_{st} \tag{1}$$

$$\text{employment growth}_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_t + \psi_s \cdot t \; + \epsilon_{st} \tag{2}$$

$$\ln(\text{employment})_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_t + \epsilon_{st} \tag{3}$$

$$\ln(\text{employment})_{st} = \beta \cdot \ln(\text{MW})_{st} + \phi_s + \tau_t + \psi_s \cdot t \; + \epsilon_{st} \tag{4}$$

Equations (1) and (3) do not include state time trends, while equations (2) and (4) do. In each Monte Carlo repetition, we store the point estimate for each β in each of these specifications, ignoring the standard errors.

A.2.3 Results of Simulation Exercise

We conducted 10,000 repetitions of the above steps, which yields 10,000 point estimates for each β value in equations (1) - (4). In Table 4, we present coefficients at the first percentile, the median, and the 99th percentile of these distributions.[23]

Table 4: Effects of the Minimum Wage on Simulated Employment Outcomes

Coefficients:	1^{st} Pctl.	Median	99^{th} Pctl.
Job growth			
[0] Simulated true effect	-.0505	-.05	-.0495
[1] Estimate without linear trends	-.0669	-.05	-.0332
[2] Estimate including linear trends	-.0752	-.0502	-.0244
Log of employment			
[3] Estimate without linear trends	-.659	-.446	-.227
[4] Estimate including linear trends	-.141	-.0248	.09035

Notes: Estimated coefficients result from regressing the outcomes (in rows) on the log of the minimum wage in the simulated data. Reported values are at the first percentile, the median, and the 99th percentile from a Monte Carlo simulation of 10,000 repititions. The true elasticity for growth is *Normal*(-0.05,0.02) by construction in the simulated data.

Row [0] of the coefficients in Table 4 presents the true effect of the minimum wage on job growth by construction in the simulated data. Note that because we draw the coefficient from a $Normal(-0.05, 0.02)$ distribution, there is a standard error on the coefficient, but the resulting distribution of coefficients for the true effect is compact. Row [1] corresponds to Equation (1) above, relating the minimum wage to job growth in a specification without state time trends. The median coefficient is identical to the true simulated effect, and we can rule out an estimated null effect of the minimum wage on simulated job growth with high confidence. We add a state time trend to the specification in Row [2], but again the median coefficient is nearly identical to the true effect.

Results for the employment level are presented in Rows [3] and [4]. Because of the randomization that was used in generating the simulated data, there is no "true" coefficient for the effect of the minimum wage on employment in these simulated data; the exact extent to which the effect on job growth is reflected in the employment level depends partly on the (random) ordering of the simulated changes to state minimum wages. However, given that in this simulation the minimum wage has a large negative effect on job growth by construction, it is reasonable to expect a fairly large – and certainly non-zero – effect on the employment

[23] The full code used in this simulation, along with all other code and data included in this study, is available from the authors online and by request.

level. This is indeed the case in Row [3], which omits state time trends. But, including state time trends in Row [4] attenuates the estimate to a (small and statistically insignificant) zero. This attenuation occurs despite: (1) a large true effect on job growth by construction; and (2) no systematic correlation of changes to state minimum wages with state effects, year effects, or state time trends.

B Historical Minimum Wage Increases and Erosion

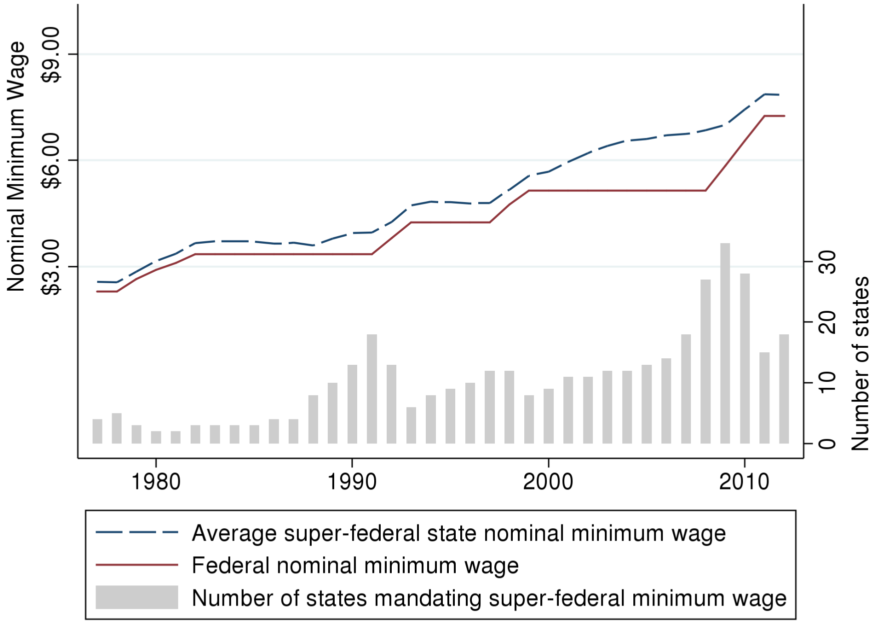


Figure 5: Comparison of federal to state nominal minimum wages

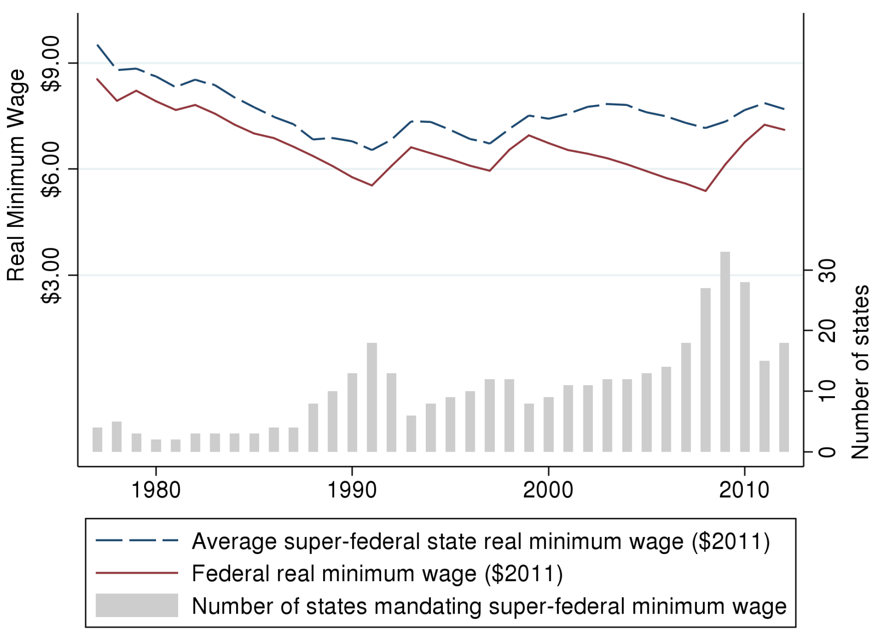


Figure 6: Comparison of federal to state real minimum wages ($2011)

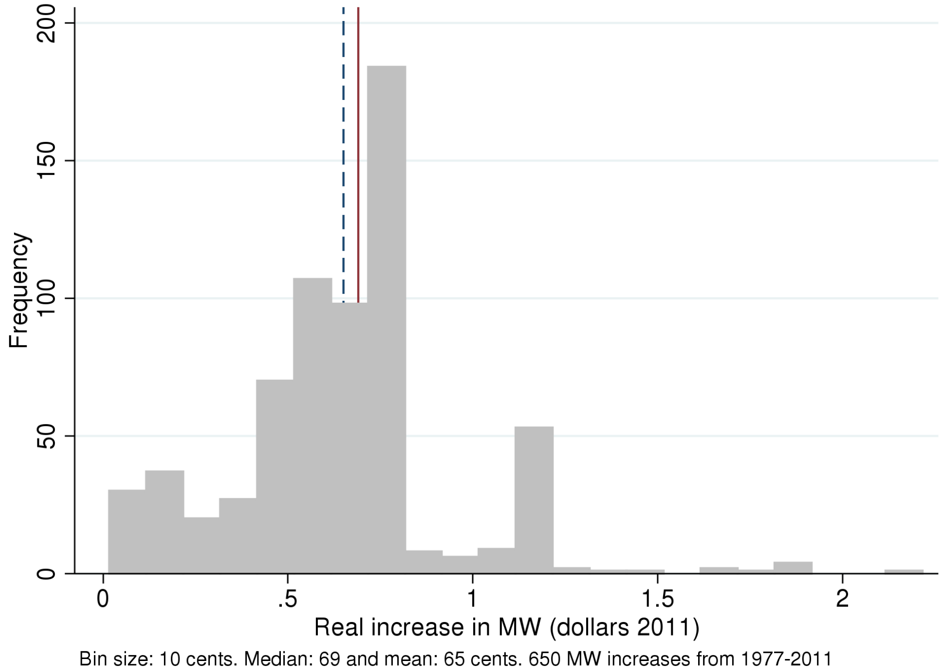


Figure 7: Distribution of real minimum wage increases

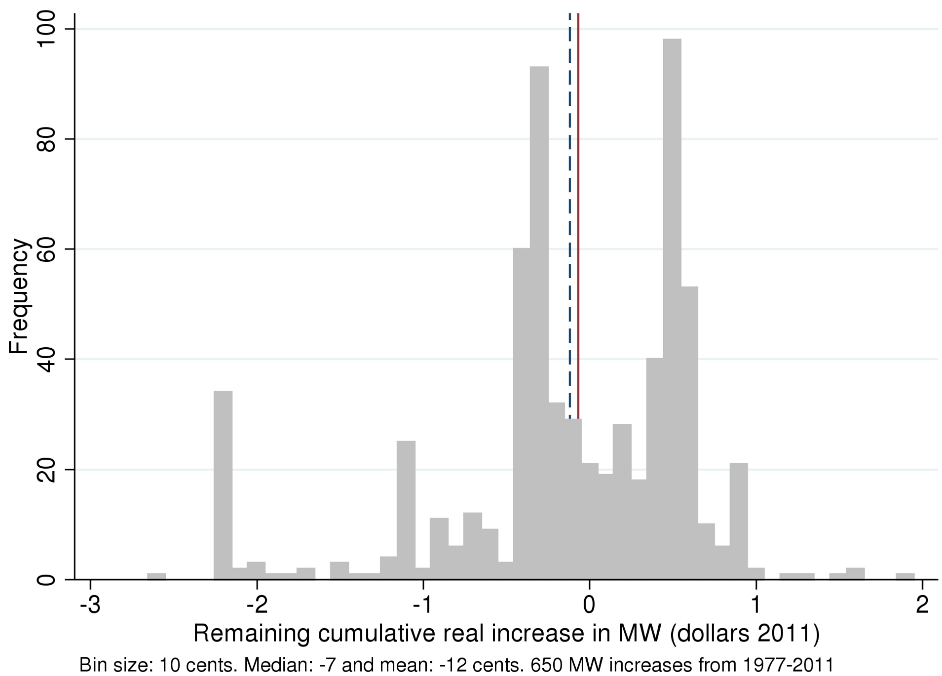


Figure 8: Cumulative difference in real minimum wage prior to a new increase

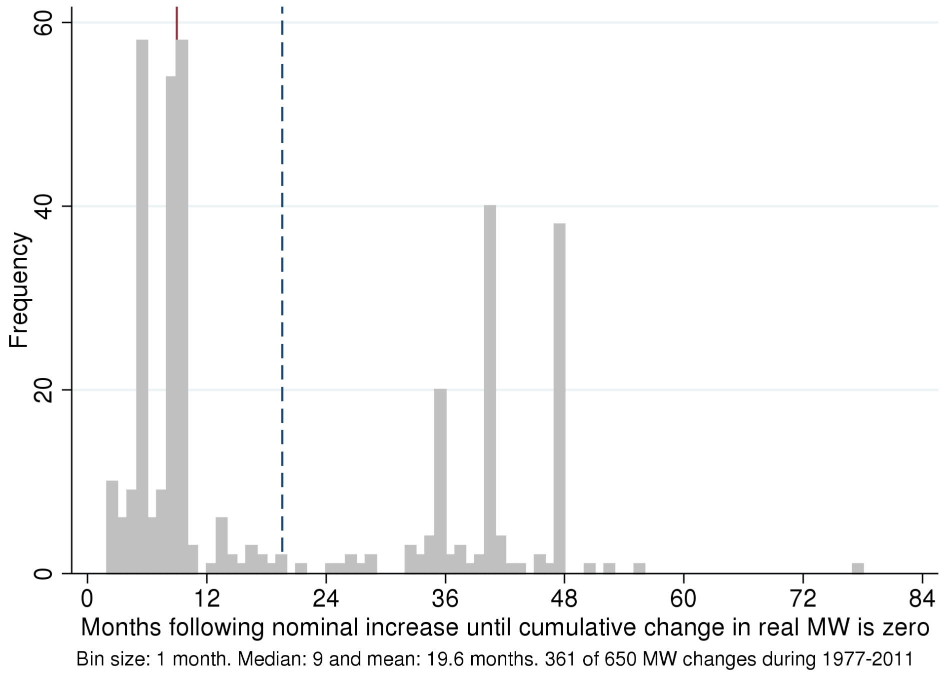


Figure 9: Erosion of real increases in minimum wage

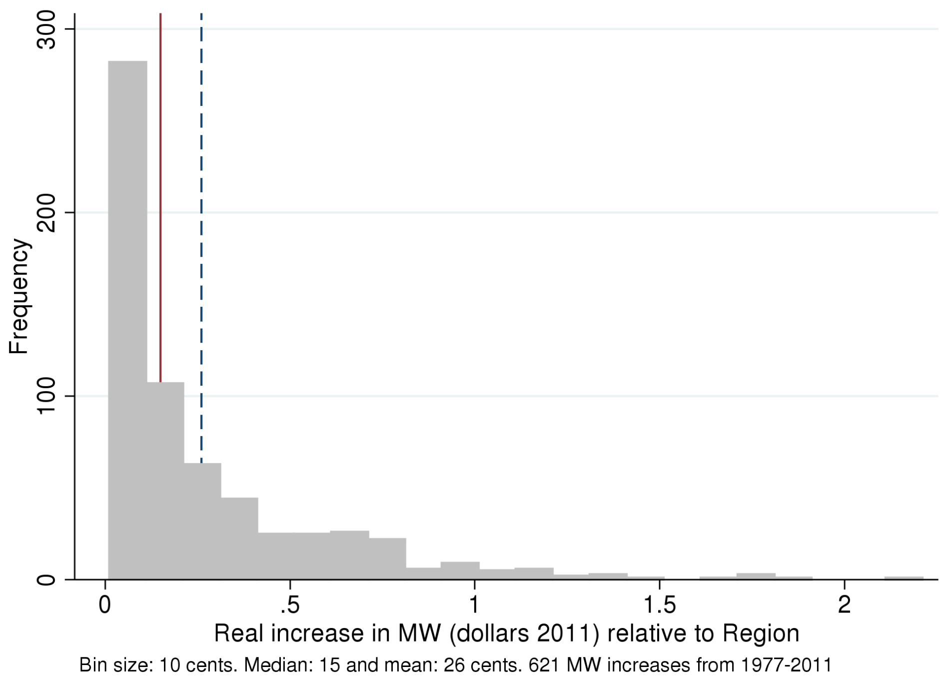


Figure 10: Distribution of relative minimum wage increases

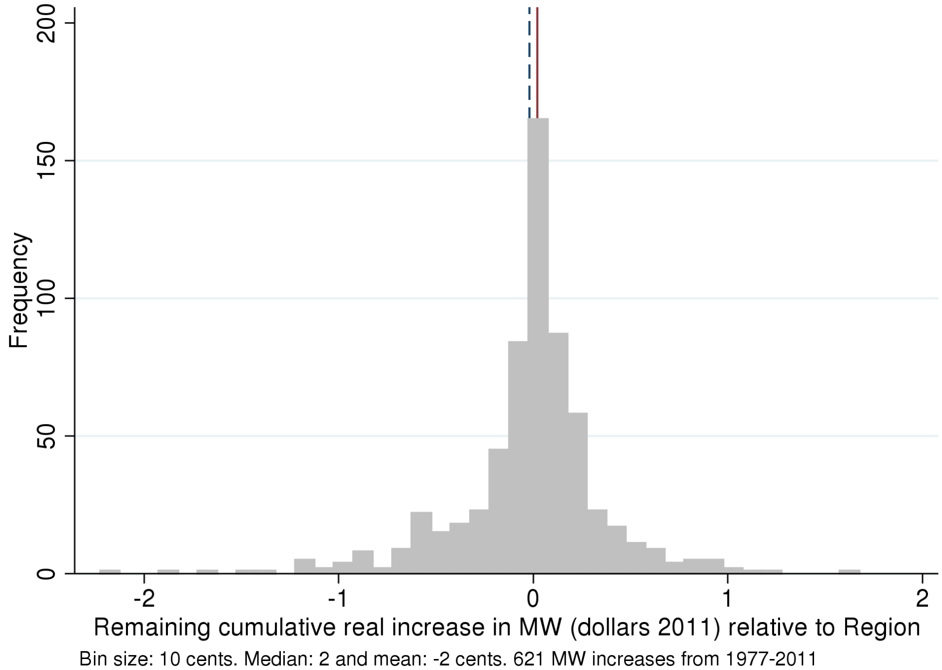


Figure 11: Cumulative difference in relative minimum wage prior to a new increase

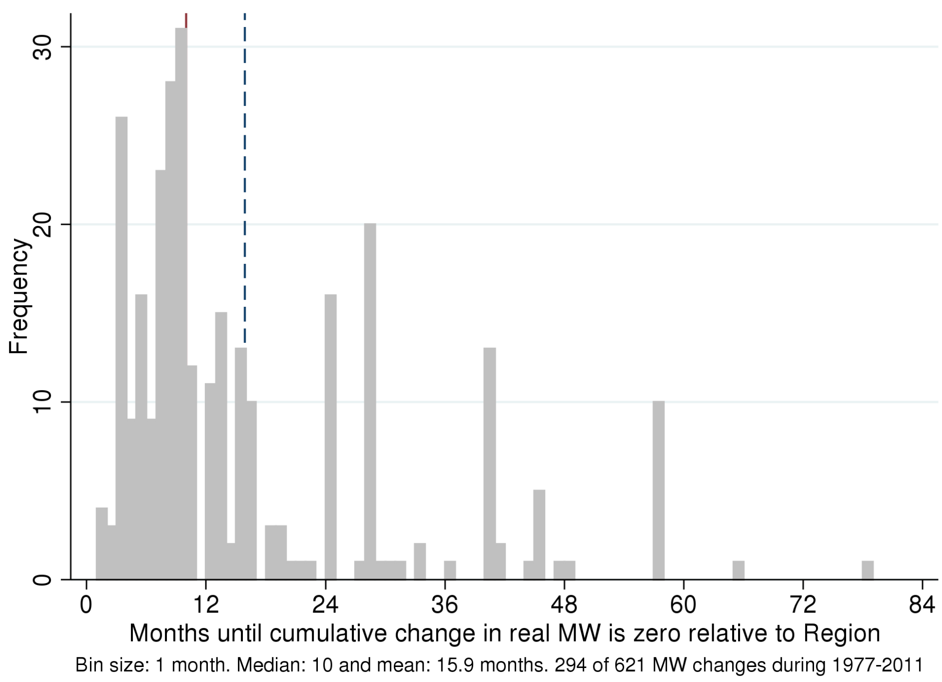


Figure 12: Erosion of relative increases in minimum wage